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ON THE INTERPRETATION OF TEMPERATURE MEASUREMENTS MADE AT HIGH LEVELS

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[Weather Bureau Airport Station, New York, N. Y., January 1940]

INTRODUCTION

The continued and increasing interest in, and the progress being made in, the extension of meteorological observations to very high levels makes it desirable to extend our knowledge of the accuracy of those measurements. This is particularly true in the case of temperature measurements because there are sources of error in them which have not yet been satisfactorily evaluated. These causes of error are chiefly (*a*) the effect of insolation if the observations are made during the daytime, (*b*) the effect of radiation from the instruments during night observations, and (*c*) the effect of lag in the temperature elements. In this study it was attempted to evaluate the magnitudes of these effects in the sounding balloon data obtained by the Weather Bureau during the International Month July 1938, at Omaha, Nebr. A general description of sounding-balloon technique will be found in (1), and more recent details regarding the balloons used and the arrangement of equipment in (2).

The program of observations during the month of July 1938 included the release of 1 balloon daily about 90 minutes before sunset, 9 others on separate days immediately after sunset, and 10 more at the time of the regular aerological sounding on days different from those on which the post-sunset observations were made. The observations begun daily about 90 minutes before sunset will hereafter be referred to as the 6:00 p. m. observations, those begun immediately after sunset as the 8:00 p. m. observations, and those made in the early morning as the 2:00 a. m. observations. The actual times of observation ordinarily differed from these times by a half hour or less. Each balloon released carried both a Fergusson (3) and a Jaumotte (4) meteorograph with the exception of 1 which carried only a Fergusson meteorograph. The Jaumotte meteorographs used were of a type manufactured in this country, and the pressure mechanisms were so faulty that none of the pressure records can be relied upon. The temperature elements performed satisfactorily, however, so that the temperatures recorded by the 2 types of instruments can be compared at points which can be definitely identified as being synchronous on the two traces. Hence, the instrument chosen as the standard was the Fergusson type; and all of the data referred to were obtained from the records of those instruments except where specifically mentioned. Two-theodolite observations (5) of the balloons were made whenever the weather permitted. As a result of good weather, the use of a fairly high rate of ascent and the existence of relatively light winds, all but 10 of the balloons released at 6:00 p. m. were followed to the bursting point with 2 theodolites. The theodolite observatious served as a check on the altitudes computed from the meteorograph records and were used as the standard in a few cases to correct the instrumental pressure records at very high elevations. An account of the winds obtained from the 2-theodolite observations will be found in (2).

The object in making the night flights, of course, was to obtain data for use in studying the effects of insolation and radiation on the instrument and in studying the possibility of a diurnal variation of temperature at high levels. The 6:00 p. m. observations differed, on the average, by about 90 minutes in time from the 8:00 p. m. flights and it was thought at the time the observations were made that any diurnal variation of temperature during this interval would be small, so that any observed differences in temperature between 6 and 8 p. m., at high levels, could be explained as being due to the sum of the effects of insolation and radiation. It was then expected that this effect could be removed from the 6 p. m. records so that the diurnal variation in temperature between 6 p. m. and 2 a. m. could be determined. The use of two different types of instruments which were not expected to be subject to the same insolation and radiation effects was expected to furnish further evidence as to whether the observed temperature changes were real.

INSOLATION AND RADIATION EFFECTS

The term radiation should properly include the effects of insolation by day and radiation at night, since both are radiation phenomena. However, for convenience of reference, the term "insolation effect" is used to indicate the effect on the recorded temperatures of the sun's radiation striking the meteorograph; and "radiation" is used to indicate the excess of outgoing over incoming radiation at night. The effect of insolation in causing the meteorograph to record a temperature higher than that of the ambient air stream depends upon the lag coefficient of the temperature element, the rate of ascent of the instrument, the density of the air stream, the absorption coefficient of the instrument case and radiation shielding, and upon the construction of the instrument (6), (7), (8), (9). It is difficult to evaluate theoretically for a given instrument the effects of the latter two of these factors so that the absolute error can be obtained. For this reason there is a definite advantage in having night observations to compare with day observations in studying the effect of insolation.

The first three of the above-named factors, namely, lag coefficient of the temperature element, rate of ascent, and air density are involved in the determination of the total insolation effect in the following manner: The smaller the lag coefficient, the nearer the instrument will record to the temperature of the passing air stream, considering lag alone. Air is flowed past the temperature element by the rise of the instrument so that rate of ascent is a measure of the rate of flow. It is assumed that the instrument case and radiation shielding will absorb a certain amount of insolation and be at a somewhat higher temperature than the surrounding air. Hence, it is to be expected that if the rate of flow of air past the element is low enough some preheated molecules of air will reach the temperature element through turbulence within the ventilating tube. Radiation and conduction from the warmer case and shielding will further affect the temperature element. The net result, then, will be that the instrument will record temperatures higher than those in the undisturbed air. Obviously, the greater the rate of ascent the less the air stream will be heated by conduction and the more the instrument will be cooled by the air, so that a high rate of ascent reduces the effect of insolation. On the other hand, the rate of ascent cannot be made too great for the reason that the lag of the temperature element may then become important, since, in general, the temperature will be changing with altitude, and significant changes in the vertical temperature gradient will not be recorded by the instrument. The air density is an important factor since it enters with the velocity in determining the rate of removal of heat from the temperature element. The density decreases rapidly with increasing altitude while the rate of ascent remains substantially constant. Hence, the effective ventilation likewise decreases rapidly with increasing elevation, and it is to be expected that when it becomes small enough the instrument will begin to record too high temperatures. A given type of instrument having a given rate of ascent and ascending at a given time of day relative to sunrise or sunset should, then, begin to record a given amount of insolation effect at the same height every day, under not too different meteorological conditions.

During the Second International Polar Year a large number of sounding-balloon observations were made in this country. These observations were made in series, each consisting of three flights. The flights in each case were made near noon, midnight, and 6:00 a. m., C. S. T., thus covering an 18-hour period. These observations were studied (6) in an effort to determine the insolation effect on the Fergusson meteorographs and results were obtained which became quite significant at comparatively low elevations (see figure 1 of (6)). What was believed at that time to be insolation effect became important at about 10 km. in the case of the 6:00 a. m. flights and at a less definitely marked but much lower elevation in the case of the noon flights. The effect was larger at noon than at 6:00 a. m.

Dines (7) made a similar study about the same time of the insolation effect on the Dines (7a) meteorograph, using a different method entirely than that used in the above study. The time of his day flights corresponded closely to the time of the Weather Bureau's 6:00 a. m. Polar Year observations and the two sets of results were almost identical. Both investigators reached the same conclusion regarding the mechanism by which the effect was produced, namely, the direct absorption by the temperature element itself of solar radiation after multiple reflection in the ventilating tube. These instruments are constructed similarly in this respect. The point of difference in the two investigations was that, as mentioned above, the results obtained by Ballard indicated a larger insolation effect near noon than near 6:00 a. m., while Dines saw no indication that the effect on the Dines meteorograph had a diurnal variation.

During the spring and early summer of 1933 Jaumotte (8) obtained a number of records up to high levels which showed large inversions beginning some distance above the tropopause. Mainly from a study of ascent and descent records and critical data regarding the physical characteristics of the Jaumotte meteorograph he was able to show that the errors in the recorded temperatures caused by insolation were negligible even at very high levels, i. e., 26 km. or so. Jaumotte has since (10) extended his work and still concludes that there is no important insolation effect in the records obtained by him with his meteorograph. His conclusions are arrived at somewhat indirectly, though, instead of by observation and, hence, are not entirely convincing. He made a number of night

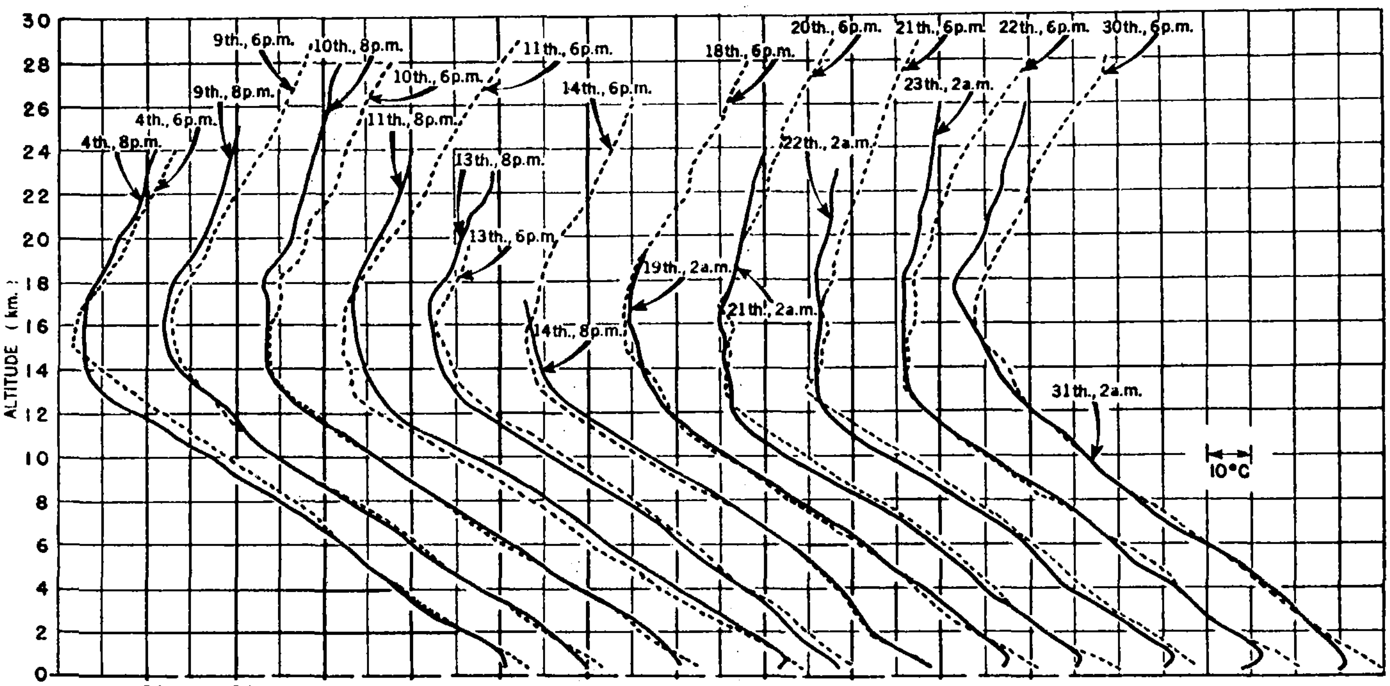

FIGURE 1.—Temperature (abscissa) against height above sea level (ordinate) for dates and times indicated on each curve. The −50° C. abscissa for each date is indicated near the curve for that date. Abscissa lines are 10° C. apart.

flights and in particular made at least one series of observations extending throughout a day and night, this latter to study the diurnal change of temperature. Unfortunately, something happened to the balloon or instrument in nearly every case so that no conclusions could be drawn from the night flights except that the inversion observed was real because the night flights did show an inversion at high levels.

On account of the construction of the Jaumotte meteorograph the effect of insolation could not operate in the same manner on this instrument as it was believed by Dines and by Ballard to operate on the Dines and Fergusson instruments respectively. For this reason and on account of the difference of materials composing the cases of the Jaumotte and Fergusson meteorographs, it would hardly be expected that the effect of insolation on the temperature records of the two instruments would be almost identical unless the effect on each is negligible. It is more or less surprising, therefore, to find that the July 1938 observations show almost identical temperature records to have been made by the two instruments. That is, the temperatures themselves differed, often by 3° or 4° C., presumably on account of errors in calibration, poor base lines, etc., but the magnitudes of the changes were almost the same. To examine the action of each instrument at high elevations, the difference between the minimum temperature recorded on the flight and that recorded at the maximum elevation was obtained for each flight on which both instruments made a good record to the maximum height reached by the balloon. Next, the difference between the magnitude of the inversion recorded by the Fergusson and that recorded by the Jaumotte instruments was found. The average value of this difference was 1° C. for 13 day flights and the same for 5 night flights. It may be safely concluded from this that the effects of insolation and night radiation are either negligible or substantially the same on both instruments.

Figure 1 shows temperature plotted against height for each of the night flights extending well into the stratosphere for which there is a corresponding daylight flight. The daylight flight made just prior to the night flight has been plotted on the same axis as the night flight in each case so that the temperature changes level for level from 6:00 p. m. to either 8:00 p. m. or 2:00 a. m., as the case may be, can be observed directly. It should be noted that both day and night flights indicate, in some cases, layers 5 or 6 km. thick which are substantially isothermal, the day and night flights agreeing in this respect. Now, if the instruments were appreciably affected by insolation, then a recorded isothermal on a daylight flight would mean that the true air temperature was decreasing with increasing height. On account of the relative infrequency of night soundings the effect of radiation from the instrument at night to cause it to record too low temperatures is ordinarily given little consideration. However, there is no obvious reason why this effect should be ruled out. If it is appreciable, then an instrument released at night and recording an isothermal condition would indicate that the true air temperature was rising with increasing elevation. Thus, if two instruments are released, one just before sunset and the other just after, and each records an isothermal condition and it is assumed that no actual temperature change occurs, then it necessarily follows that the respective instruments were not appreciably affected by insolation and radiation up to the height of the top of the isothermal layer. This line of argument indicates that the effects of insolation and radiation were quite small up to the region of 18 km. or so at least in the case of the July observations. Obviously, these two effects are in opposite directions so that any observation of the effect is of the sum of the two effects, and an observation of no effect means that each is negligible.

The above argument did not include the effect of lag alone. In the two cases discussed the effect of lag would be to cause the night instrument to record temperatures which were too low and the day instrument temperatures which were higher than the true values. Since it was concluded that the true lapse rate was isothermal then the effect of lag would be zero.

Above the approximately isothermal layer, and beginning on the average at about 20 km. the temperatures as recorded in available soundings increase with increasing height at the rate of about 2.5° C. per km. for the day flights and somewhat more slowly for the night flights. The effect of lag in this region then is to cause the instruments to record too low temperatures, since the instrument is always colder than the air through which it is passing. The effect of radiation is in the same direction while that of insolation is in the opposite direction. Hence, if all three of these effects were important, it might easily be true that the day-time temperatures were nearer the true values than those recorded at night. The effect of lag should be approximately the same on the day as on the night flights (at least if the lapse rates are not radically different) so that any average differences would be expected to be due to the sum of radiation plus insolation effects or to a diurnal variation of temperature.

VENTILATION

Ventilation of the temperature element, as stated above, is accomplished through the ascent of the instrument. Its magnitude will thus depend upon the rate of ascent, level for level, at high elevations, since the density, level for level, does not in general at any time and place differ by more than 3 percent from a mean value at levels between 20 and 30 km. On the other hand, the rates of ascent may vary between flights by as much as 30 percent. It was thus thought that the temperature changes observed from day to day might be explained as being due partly to changes in the rates of ascent, high temperatures being recorded when the rate of ascent was low and lower temperatures when the rate of ascent was higher. However, no such correlation could be found for the July series of observations. The rate of ascent used during this series averaged about 75 percent greater than that used during the Polar Year so that from this standpoint the July records would be expected to begin to show appreciable insolation effects at higher levels than the Polar Year series.

Figure 2 shows the mean temperature change from 6 p. m. to 8 p. m. and 6 p. m. to 2 a. m. plotted against height, based on all the available flights. The original temperature height curves were smoothed somewhat before the temperature changes were obtained in order to eliminate, insofar as possible, the effects of horizontal differences in temperature on the different flights and small errors in altitudes. It will be noted that the mean changes from 6 p. m. to 8 p. m. and from 6 p. m. to 2 a. m. were about a degree or less at all upper levels between 2 and 16 km. Beginning at 16 km. the daylight flights indicated higher temperatures than the night flights, the differences increasing with altitude. There is some evidence that the average change at high levels is somewhat greater between 6 p. m. and 2. a. m. than it is between 6 p. m. and 8 p. m., which would be expected to be the case if there is a diurnal variation of temperature at these

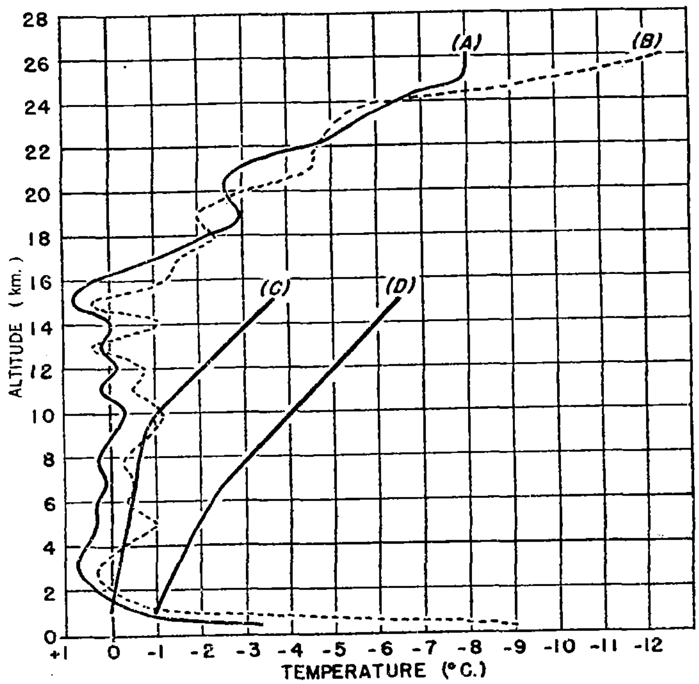

FIGURE 2.—Recorded temperature changes in ° C. against height for: A, July 1938, 6 p. m. to 8 p. m.; B, July 1938, 6 p. m. to 2 a. m.; C and D are Polar Year data reproduced from Mo. Wea. Rev., Feb. 1934, 62: 45-53.

altitudes in phase with that at the ground. The number of observations upon which the means are based, however, is too small for the evidence to be conclusive.

It will also be noted that the shapes of the two curves are quite similar. Since the two sets of data cover different periods it would hardly be expected that the curves would accidently run roughly parallel, changing slope at the same altitudes in a number of cases. Their shapes indicate changing insolation effect, or diurnal temperature variation with altitude. It is not safe to conclude that the changes of slope are real, however, because of the small number of observations. The temperature change from 6 p. m. of the 20th to 2 a. m. of the 21st (see fig. 1) was considerably smaller than on the other days and if this observation were excluded from the data on which the means are based, then the means themselves would be larger for the upper levels. This would support the evidence for a diurnal variation of temperature but the discarding of this observation is hardly justified. It will be shown later, however, that the temperatures at high levels at 6 p. m. on the 20th and 21st were low and rose considerably by 6 p. m. of the 22d.

It might also be argued that the temperatures at 6 p. m. on the 22d and 30th were quite high at 25 and 26 km. and that this accounts for the fact that the values of the falls in temperature from 6 p. m. to 2 a. m. for these levels are larger than the corresponding values for 6 p. m. to 8 p. m. In other words, it appears at first sight that the higher the temperature at 6 p. m. the greater will be its drop during the night, since the range in observed nighttime temperatures at the upper levels is small. This would be the case if there is an important insolation effect which has different magnitudes on different flights. The rate of ascent over the region concerned averaged about 340 meters per minute on the flight of the 22d, practically the same on the 30th and about 320 meters per minute on the flight of the 21st, the day on which the recorded temperature was lower than it was on the 22d and 30th. On the 10th, the single observation upon which the temperature change at 26 km. from 6 p. m. to 8 p. m. is based, the rate of ascent was about 400 meters per minute and the temperature recorded was 2° warmer than on the 21st with a rate of ascent of 320. At 23 km. the recorded temperature on the 10th was 1° warmer than on the 21st and the temperature difference between 8 p. m. of the 10th and 2 a. m. of the 22d was zero. These facts suggest that the insolation effect is independent of the rate of ascent over the range of rates considered up to altitudes of 26 km. or so.

Zistler (**9**) suggested that the insolation effect on a particular instrument of a given type will depend to some extent on the surface condition; i. e., degree of highness of the polish of the case and radiation shielding. To all outward appearances there was little difference in the surface condition of the meteorographs used in this series of observations and it is not believed that the insolation effect varied appreciably from instrument to instrument on account of this factor. Dines (**7**) suggested that the radiation reflected from cloud tops may be instrumental in producing insolation effect since if the sun has the proper elevation angle considerable radiation may be reflected vertically upward in a direction from which the temperature elements are not radiation shielded. This factor may vary from flight to flight depending upon the time of day and the cloudiness. It is not believed, however, that this was of appreciable importance in determining the insolation effect during the July series since the observations were made so late in the day.

If we assume for the moment that the insolation effect was substantially constant over the range of ascensional rates used during the July series, then the question arises whether the difference in rates used in this series and that used in the Polar Year series will account for the difference in insolation effect which was found. From the standpoint of elevation of the sun the 6 a. m. observations of the Polar Year are roughly comparable with the 6 p. m. observations of the July series. For convenience of reference the curves shown in figure 1 of (**6**) have been added to figure 2 of the present paper. These curves were originally extrapolated to 20 km. but the reproduction here has been extended to 15 km. only, since the data above 15 km. were extremely few. The means at 16, 17 and 18 km. did not fall smoothly on the curve as originally drawn and the possibility exists that the curves may change shape radically in this region.

The average rate of ascent during the Polar Year was about 220 meters per minute while the average for the July series was about 350. If the curves shown in figure 2 indicate insolation effect due to insufficient ventilation, then the effect begins to be significant at about 10 km. for the Polar Year observations (6 a. m. flights) and at about 16 km. for the July observations. If there is no change in the character of the air flow about the temperature element over the range of rates 220 to 350 meters per minute then the effective ventilation for each rate is proportional to the product of some power of the rate of ascent times some power of the air density. According to Jaumotte (**8**) the power should probably be three-fourths in both cases. Using this power we find that the effective ventilation during the Polar Year at 10 km. was 40 percent greater than that at 16 km. during the July series. If, as Jaumotte also suggests, we use the three-fourths power of the density and the one-half power of the rate of ascent, then the effective ventilation was about 60 percent greater during the Polar Year. The ventilation on the noon flights of the Polar Year at altitudes with corresponding amounts of insolation effect was much greater than on the 6 a. m. flights and still larger than that on the flights of the July series. This indicates quite clearly that either the observed differences were not entirely insolation effect or that the method used by Jaumotte (**8**) and Zistler (**9**) and others to compute the insolation effect in their records is not applicable to the Fergusson meteorograph.

The method used by these investigators is based on the record made by the meteorograph on the ascent and descent, the rates of change of altitude being much different in the two cases. It is rather difficult to evaluate a Fergusson meteorograph descent record accurately enough to determine the lag coefficient of the temprature element from the ascent and descent records because of the difficulty of synchronization at high rates of change of pressure. A few such records were checked roughly in an effort to find whether there were important and consistent temperature differences between ascent and descent near the tops of the flights. As well as could be determined they were quite small, i. e., apparently not more than one or two degrees. One descent record was evaluated in considerable detail and as accurately as possible. This was the record made on the 28th of July and it was chosen because the record was quite clear, because the temperatures recorded on this flight in the upper levels were higher than on any other flight during the month and because the rate of ascent was somewhat lower than that of any other flight during the month. The results are shown in figure 5 where the ascent and descent temperatures have been plotted against altitude. The rate of ascent on this flight averaged about 290 meters per minute and the rate of descent varied as indicated in the figure. It will be noted that both ascent and descent curves have substantially the same slope and are spread apart in the way in which they should be spread if there is lag in the temperature element. That is, in the inversion the ascent temperatures are lower than the descent temperatures, while in the region in which the temperature was decreasing with increasing height the reverse is true. It is very interesting to note that the effect of lag alone seems to predominate—i. e., that the effect of insolation is relatively small. This is evidenced by the fact that the descent curve did not show a sudden drop in temperature just after the descent began to a value below that recorded on the ascent as would be the case if there had been a large insolation effect in the ascent record.

Further evidence that the observed temperature differences were not caused by insolation effect is the fact that with this much slower rate of ascent during the Polar Year the actual temperatures recorded on the few summer flights which reached 20 km. and higher were not substantially, if any, higher than those recorded during the July series. Furthermore, there were flights made during the Polar Year at noon, having this lower rate of ascent, on which the temperature recorded was nearly isothermal for more than 10 km. above the tropopause. These records were obtained during the winter season, however, and the evidence indicates that the upper inversion, if it is a permanent winter feature, begins at a higher altitude in winter than in summer (**11**).

DIURNAL VARIATION OF TEMPERATURE

The evidence thus favors the interpretation of the observed temperature changes during the Polar Year as actual diurnal changes rather than insolation effect. Dines' contention that the insolation effect should not be substantially greater at noon than at 6 a. m., as well as the evidence just presented regarded the relative independence of the "error" of the rate of ascent, and the agreement between the Fergusson and Jaumotte meteorograph temperature records all support the argument that there is an appreciable diurnal temperature variation at all levels up to 26 km. or higher and that the amplitude of the diurnal variation increases with increasing altitude from 2 or 3 km. upward. The Polar Year and the July data are not strictly comparable because the former are based on flights distributed throughout the year while the July data cover only a short period during which the cloudiness and amount of convection probably differed materially from the normal annual values. Hence, the apparent disagreement between the two sets of data is not necessarily a disagreement in fact, if the differences are true and diurnal.

It has been attempted by several investigators to explain the temperature distribution in the stratosphere by considering a condition of radiative equilibrium between the ground, atmosphere, space, and the sun. Pekeris (**12**) made a critical survey of the more important theories regarding the heat balance of the atmosphere and it is quite clear that none of the theoretical treatments have successfully explained the observed distribution of temperature at very high levels. In particular, none have explained the inversion of temperature observed to begin shortly above the tropopause.

It is often assumed that the only gas at 20 to 30 km. which is present in important quantities and which absorbs solar radiation in significant amounts is ozone. Two papers (**13**) and (**14**) summarize very well the present knowledge of the distribution and absorption qualities of ozone. Obviously, this knowledge is very incomplete but according to Gowan (**15**) and Penndorf (**16**) the rate of heating or cooling of the air at levels between 20 and 30 km. due to absorption and emission by ozone is much too slow to account for an appreciable diurnal variation of temperature at these levels. A point which has not been made entirely clear, however, is what happens at sunrise and sunset. Wulf and Deming (**17**) have shown that the rate at which ozone equilibrium is attained at these levels, under the influence of solar radiation, is extremely rapid, being of the order of minutes. Whether the disturbance of this equilibrium together with the absorption and radiation of other constituents of the atmosphere at these levels can account for a sudden drop of temperature just after sunset at these levels and arise just after sunup is not known by the author of this paper. Nevertheless, it does not appear that the available evidence is conclusively contradictory to the operation of such a process. In this connection it is interesting to note the work of Maris (**18**) who, apparently basing his assumptions mainly on absorption and radiation by carbon dioxide and water vapor, arrived at the conclusion that a diurnal variation of temperature in these levels does exist. Furthermore, the values he gives for the magnitude of the variation from day to night in summer agrees reasonably well with the July data shown in fig. 2. His conclusions regarding the difference between a winter day and night are also supported to some extent by the scanty data available.

INTERDIURNAL TEMPERATURE VARIATION

Figure 3 shows temperature plotted against day of the month, from the 6 p. m. observations, for the ground at the time of release of the balloon, and for the standard levels indicated. In addition, the daily maximum surface temperature and the temperature at the level where the vapor pressure was 10 mb. have been plotted. The daily

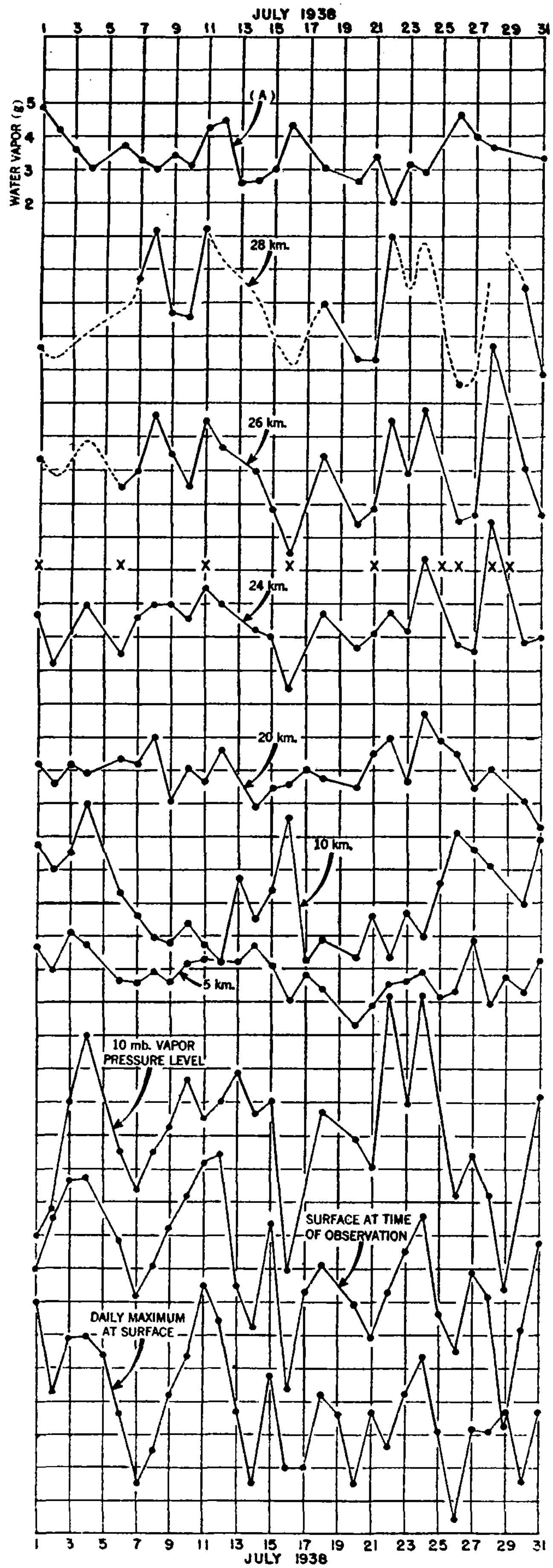

FIGURE 3.—Temperature (ordinate) against date from 6 p. m. observations at levels indicated on each curve. Curve A shows water vapor in grams per unit column 1 square cm. in cross-section from ground to 500 mb. pressure level plotted against date. X between 24 and 26 km. curves indicate cloudiness greater than 5 tenths at time of observation.

maximum temperatures used were those published on the daily Washington weather map. The temperatures at the levels at which the vapor pressure was 10 mb. were obtained directly from the soundings. At the top of the figure the amount of precipitable water in a column one square centimeter in cross section and extending from the ground to a pressure of 500 mb. has likewise been plotted against the day of the month. These values were obtained by integrating the area under the vapor pressure curve between the ground pressure and 500 mb. with a planimeter. In figure 4 the logarithm of the temperature at 24 km. has been plotted against the logarithm of the temperature of the 10 mb. vapor pressure level for all the days during the month for which data were available and on which the amount of cloudiness at the time of release of the balloon was five-tenths or less.

It will be noted from fig. 3 that the day-to-day temperature variations at 5 km. were relatively small and roughly in the same direction as the surface temperature changes. Below this level the day-to-day changes were larger as was also the case for levels higher than 5 km. The day-to-day changes in the troposphere above 5 km. reached a maximum value in the region of 10 km., at which level, as would be expected, they were opposite in sign, in general, to the surface changes. The day-to-day changes again reached a minimum value in the neighborhood of 20 km. and above this level they increased in magnitude with increasing altitude, and, in general, the sign of the change corresponded to that of the surface temperature. At 24 km. and higher the daily variations agreed about as well with the daily surface maximum as with the surface temperature at the time of the flight. It was attempted to correlate the daily variations at 24 and 26 km. with some temperature at a lower level because the solar radiation at these high levels varies little from day to day at a given time of day. The reason for choosing a level of constant vapor pressure was that, on account of the relative uniformity in shape of the vapor pressure curves during the series, it appears likely that some such surface will constitute some sort of effective radiating surface. The value 10 mb. was purely arbitrary.

The data support this idea to some extent as will be apparent upon inspection of the curves. When the curve of total amount of water vapor in the lower levels is considered along with the amount of cloudiness, the evidence is still better that the temperatures and the temperature changes at the high levels are real and are radiative equilibrium values of some sort. The days on which the amount of the sky covered with clouds at the time of the observation was six-tenths or more have been indicated with an "X" between the curves for 24 and 26 km., except when the clouds were very thin cirrus or cirrostratus through which the balloon could be seen with the unaided eye. It would not be expected that there would be a simple relation between the temperatures at high levels and those at low levels, as a result of radiation exchange, when there is a widespread heavy layer of clouds at intermediate levels. The top of this cloud layer would in this case probably constitute the effective radiating surface. It is not entirely clear just how this would affect the temperature at high levels and it is somewhat instructive to consider the cases of the 16th and 28th of the July series. On those days, respectively, were recorded the lowest and highest temperatures at 24 and 26 km. which were observed at these levels during the month. Both days were cloudy. Lower clouds obscured the sky at the time of the observation on the 16th so that the presence or absence of heavy upper clouds could not be determined. A well-defined cold front passed the station between the morning of the 16th and the morning of the 17th and the surface temperature at the time of the ob-

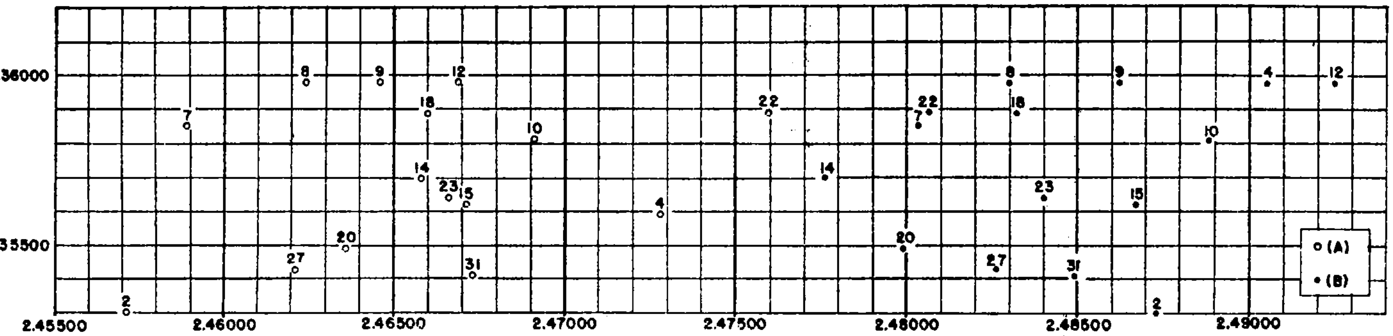

Figure 4.—Logarithm of Absolute temperature at 24 km. plotted as ordinate against: (A), logarithm of Absolute temperature at the 10 mb. vapor-pressure level; (B), logarithm of Absolute temperature at ground at time of observation. Numbers refer to the date.

servation on the 16th was quite low. In this case it is quite probable that heavy clouds extended to high levels and that their low upper surface temperature was effective in reducing the temperatures at higher levels. On the 28th eight-tenths of the sky was covered by altostratus clouds. Naturally, the top of this cloud layer had a temperature much lower than any temperature during the month at the 10 mb. vapor pressure level. In fact, the temperature at the top of this cloud layer was probably in the neighborhood of −25° C. At first thought this would appear to indicate that the recorded temperature at the high levels is entirely unreliable. However, it may easily be true that the top of a cloud, which is known to be an excellent radiator (19), at this temperature and height and above which there can be little water vapor, is more effective in raising the air temperature at high levels than is a much higher temperature at lower levels operating through a blanket of water vapor. This line of reasoning as applied to the two cases of the 16th when the temperature was low at high levels and to the 28th when the temperature was high may appear inconsistent. However, it should be kept in mind that we know nothing about the existence of high clouds and the distribution of water vapor above the lower clouds in the somewhat complicated situation on the 16th.

In fig. 4 it will be seen that the scatter of the points in the graph of temperature at 24 km. against that at the 10 mb. vapor pressure level is somewhat less than in the graph of temperature at 24 km. against surface temperature at the time of the observation. It was thought that if the air at 24 km. was in radiative equilibrium with the lower levels its temperature might be found to vary as the third or fourth power, *roughly*, of the temperature at one of these lower levels. In this case when the logarithms of the temperatures are plotted as in fig. 4 a line of slope 3 or 4 should be the curve of best fit. Actually, the slopes of the lines of best fit for the two cases in fig. 4 were computed by the method of Least Squares to be zero in each instance. It should be pointed out, however, that the relative humidity records in this set of observations are quite inaccurate, so that the temperatures at the 10 mb. vapor pressure level are equally undeterminable. The amount of water vapor between the ground and the 500 mb. pressure level was considerably above the average on the 2d, 12th, and 27th, and the effect should be to make the temperatures on these days at 24 km. appear too low, if ground radiation is an important factor. The amount of water vapor below the 500 mb. pressure level on the 22d was considerably below the average for the days considered and the temperature recorded on that date, as we would expect from this reasoning, appears high. A better correlation would possibly have been found if a regression equation relating the temperature at 24 km. to both the surface temperature and the water vapor content had been worked out.

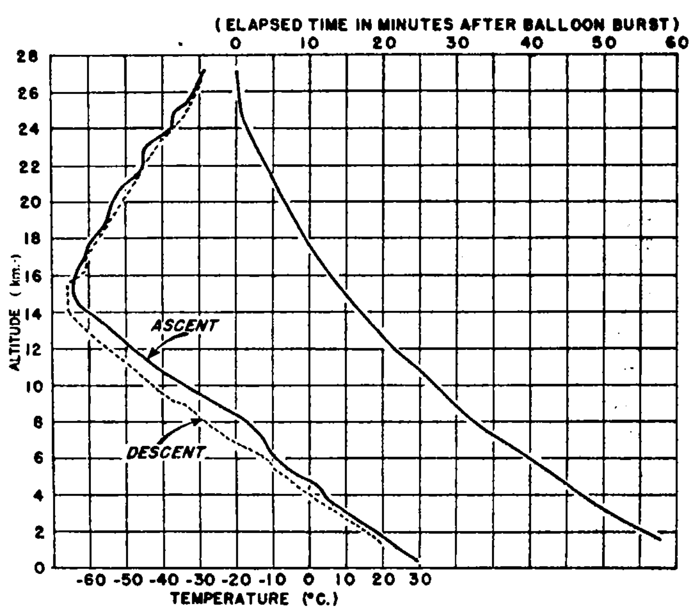

Figure 5.—Temperature in ° C. against height above sea level from ascent (solid line) and descent (broken line) records of the 6 p. m. observation on July 28, 1938, and time-altitude curve for the descent.

There can be little doubt about the existence, normally, in summer at least, of this large temperature inversion in the stratosphere. If radiation from the lower layers and the ground is a controlling factor of its cause it is not clear just how it operates. Too little is known about the composition of the atmosphere at say, 24 km. and higher, and too little is known about the absorption and radiation qualities of the gases suspected of being present, to answer the question. It seems entirely possible, however, that there is considerably more water vapor present at high levels than is at present generally assumed to be the case. The amount of water vapor in a given volume could increase quite rapidly at high levels in the inversion and its percentage would then increase still more rapidly with increase in elevation because of the decreasing total density. The temperature might then rise with increasing elevation because of the increasing absorption of outgoing radiation by the increasing water vapor content of the higher layers and its increasing ability to raise the air temperature. That the temperatures at the high levels are controlled to some extent by the ground and lower air layer temperatures is indicated to some extent by winter observations, which show lower temperatures than summer observations at levels well within the stratosphere.

CONCLUSIONS

In conclusion it may be said that the balance of evidence appears to support, but does not prove, the following statements:

1. The temperatures recorded by the Fergusson sounding balloon meteorograph up to altitudes at least as great as 26 or 28 km. are effected very little by insolation. By "very little" is meant not more than 5°C. error at 26 km. and in all probability the error is not greater than 1°C. or so. The best evidence of this is the comparison between Fergusson and Jaumotte meteorograph records, and the Fergusson meteorograph descent records of which figure 5 is an example.

2. Since the effect of insolation is small there must be an appreciable diurnal variation of temperature at all levels up to at least 26 km.

3. The day to day temperature changes at 24 km., or so, and higher are in the same direction as the changes at the ground on clear days. This is probably made possible by a changing composition with elevation in the upper levels.

4. On account of lag the recorded temperatures in the high level inversion may actually be lower than the true values instead of too high.

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THE DUAL RAINFALL REGIME OF ROSWELL, NEW MEXICO

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INTRODUCTION

Current methods of studying the seasonal incidence of rainfall at any particular station are practically all based upon the fundamental assumption that there exists at that station some sort of normal regime of which each annual record is a variant. Though it may be conceded that such variations are of great magnitude, they are usually regarded as wholly fortuitous in character and are expected to cancel each other out in the long run. A record for some 35 consecutive years is usually regarded as sufficient to effect this process, and indeed, in many parts of the world, climatologists count themselves fortunate if the body of data at their disposal is not considerably less comprehensive than this.

Strictly speaking, the distinction between record and normal should be threefold, for we ought not to confuse the normal-as-calculated with the true normal which is no more than a hypothesis. Let us briefly review each of these aspects to make sure that their essential character is not misunderstood.

1. The *annual record* in the case of precipitation data is usually presented as a series of 12 monthly totals. The employment of arbitrary and unequal calendar months instead of more uniform intervals, such as lunar months, is certainly to be regretted but to make a change now would necessitate the conversion of all past records, a thankless and, in many cases, impossible task. The important point is that daily records, even when available, *must* be grouped together to include a considerable time-span before any signs of the emergence of a regime will become apparent. The imperative nature of this consideration is often overlooked although it is no more than an expression of our daily experience of the weather. Evidence exists that in monsoon lands the necessary period should be more than a week, in Great Britain and New England it is probable that even the calendar month is scarcely adequate for the purpose. There is no mystery about this, it is simply a reflection of the fact that in tropical lands the weather may be expected to take some anticipated course with a greater degree of punctuality than is the case in more temperate latitudes. If we wish to register that tempo with maximum efficiency, we must make adjustments of the time factor accordingly.

It is well that this should be recognised even if the presentation of published data makes use of the 12 monthly totals virtually inescapable.

2. *Mean monthly values* are also frequently published but they are referred to in varying terms. In the "Climatic Survey of the United States" they are called "averages"; in "World Weather Records" they are known as "means"; whilst in publications of the British Meteorological Office they become "normals." In view of the fact that all of these are computed by the same identical method of striking an arithmetical average, this diversity of expression is unfortunate. In the rest of this paper the term "average" alone will be used in this connection.

Elsewhere it has been shown that published averages are not always the best descriptive means for use in connection with rainfall data (**1**). Reasons have been given for regarding median values as a closer approximation to normality. But, although some acquaintance with that discussion is necessary to an understanding of the methods of analysis which follow, the present argument does not hinge upon this distinction.

3. Whilst the record is the direct product of observation and the mean is the tool of the practical climatologist, the *true normal* is no more than an hypothesis, a